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GENERAL REVIEWS AND SUMMARIES

SENSATION (GENERAL)

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The problem of the elementary processes, their nature and number, seems likely to remain open so long as psychology is constructive. For a decade, the question of the alleged 'thought-element' has held the attention of the laboratories. At the same time, the 'formal' elements of perception and ideation, which accumulated a large literature in the nineties, have not been entirely neglected. A. Gelb, of the Berlin Institute, has recently written a general critique of them (2). He appends a useful bibliography of forty-three titles. Gelb reviews the *Gestaltqualität* from the time of Ehrenfels to Marty and Kreibig. He takes up a dissenting attitude toward Ehrenfels, and he rejects the *fundierte Inhalt* of the Austrians. He contends, however, for the specific character of the *relation* which he sets down as 'given' with the ordinary 'absolute' elements of sensation. It may be questioned whether the substitution of 'relational' for 'formal' elements marks an advance in empirical psychology.

Semon (3) formulates a definition of stimulus that is consonant with his doctrine of the *Mneme* and the *Engram*. Stimulus is an operation upon living organisms which 'excites' organic substance and which produces a 'reaction.' Reactions are *sensory* (the stimulus is followed by sensation) or *objective* (an organic change, such as movement, growth, or elimination, ensues). Stimulus and excitation are factored in terms of energetics and a one-to-one correspondence is found to obtain between the factors. Stimulation does not necessarily involve kinetic energy or even the presence of an energetic agent (cf. the sensation gray and the absence of light). It is enough

that the stimulus should produce an organic change that 'releases' and maintains the excitation.

Thompson found (4) that when the head was placed within the field of a powerful magnet with alternating current a weak flickering light appeared in the retinal field. The sensation was observed to be more brilliant in the dark than in the light and to effect most strongly the peripheral portions of the field. Upon repeating the experiments, Dunlap noticed (1) that the flicker was strongest when the side of the head was presented to the coil and he suggests that the effect may be due to induction currents set up in the optic pathway. He is not certain that "there is a definite *arousal* of visual sensation by the alternating field; the effect appears more like an alternate intensification and inhibition of whatever sensory process is already in progress."

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VISION—GENERAL PHENOMENA

BY PROFESSOR EDWIN B. HOLT

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After a preliminary survey of earlier determinations in wavelength of Hering's four Urfarben, these having been made by several different methods, Westphal (10) proposes the questions: how far the Urfarben can be determined by direct judgment of *pure* color-quality, and then, how far persons will agree with one another on this point. This direct method has been previously used by Donders, L. von Kries, and E. Schottelius. Westphal finds his observers able to make the specific judgment on Urfarbe 'mit ziemlicher Sicherheit und grosser Konsequenz,' although this is subject to certain cautions (p. 227, § 5). Observers differ most markedly as to the place in the spectrum of primary red and green, they differ less as to yellow, and least as to blue, locating this fairly definitely at 478.8-479.4 $\mu\mu$. One observer's yellow lay only 14-24 $\mu\mu$ away from the green of another; and the distance between the Urfarben, in $\mu\mu$, is

not the same for different observers. As the intensity of a spectrum is increased the distance between primary red and green increases, the former moving to the left, the latter to the right; but this is due to the observer's tendency to select green and red both with a slight tinge of yellow, which yellow shows out when the intensity of the spectrum is increased (p. 228). Primary blue and yellow shift very little if at all as the intensity is changed. The admixture of white light to the colored affects the position of the Urfarben just as does reduction of the physical intensity. The mixture of a person's own primary red and green looks to him yellowish instead of gray; and so too his blue and yellow look greenish. The point of the spectrum which appears neutral to the deutanopic eye appears bluish to the normal eye. Sometimes an observer reports, in locating say primary red, that he has to choose this not where the red looks *neither* bluish nor yellowish (for he finds no such point), but where it looks *both* blue and yellow in equal degree: similarly primary yellow sometimes looks both red and green. This last point bears interestingly on some contentions of Brentano.

Westphal's observers find that the extreme red end of the spectrum begins to have a bluish cast, as indeed Maxwell has described it to have. Rayleigh (9) now reports, in agreement with König and Dieterici, that this is not the case; the farthest visible portion of the red end remains purely red. From this, then, Rayleigh would probably locate his Urrot at the farthest visible wave-length, in opposition to Westphal. Rayleigh further reports that while most persons of normal vision (himself included) see a reddish cast in the violet end of the spectrum, yet not a few see nothing here which at all resembles red; and this abnormality, he finds, is not connected with color-blindness. He drops this interesting hint (p. 205): "That ordinary normal vision is very approximately trichromic cannot be doubted; but a question may be raised as to the possible existence of a very subordinate fourth element of color."

Dittler, Orbeli, and Richter take up three further points connected with earlier investigations of Hering. Dittler and Richter (2) confirm, by a new method, the statement of Hering and Brückner that if a part of the retina (either foveal or extrafoveal) is fatigued for white light, a colored stimulus is seen on this spot less intense but more saturated than on an unfatigued part of the retina. If the intensity of the colored stimulus is increased on the exhausted portion until it here looks as *bright* as the original intensity on the unexhausted portion, it will also still look very much more *saturated*.

The two sensations can be made much more nearly (and indeed practically) identical if on the white-fatigued portion the intensity of the colored stimulus be left unchanged and white light be added to the colored until the sensation from this field is as bright as the sensation from the colored stimulus alone on the other, unfatigued part of the retina. The experiment is thought to show, as against the Helmholtz theory, that the white visual process is fatiguable independently of any color process, and that the former is therefore independent of any color process.

Dittler and Orbeli (3) test once more Fechner's assumption that excitation is proportional to stimulation. If this is true of vision it should follow that if the stimuli for two unequally fatigued spots of the retina have had their intensities so adjusted that they yield equally bright sensations, these sensations should remain equal in spite of changes in the absolute intensity of the two stimuli so long as their relative intensities remain unchanged. This hypothesis has been reported verified, and is now called by J. von Kries the 'Koeffizientensatz' (cf. *Nagel's Handbuch*, 3, 211) and by Wirth the Fechner-Helmholtz theorem. In 1873 Hering reported an experiment which seems to disprove this theorem, and the present authors confirm Hering. They find that brightness equations, established as above described, vary appreciably if the absolute intensity of the two stimuli is increased or decreased, by say 25 or 30 per cent., although the relative intensities are undisturbed.

When the eye is stimulated briefly by a moving light, a positive like-colored after-image is seen between the primary image and the so-called Purkinje after-image. If a system of two moving lights is used (as say two openings in an opaque movable screen), the conditions can be so regulated that this positive after-image of the foremost light coincides with the primary image of the second light. Three images are then seen following one another in space: (1) a primary image of the first light; (2) an image formed from the superposition of the positive after-image of the first light and the primary image of the second light; (3) a positive after-image of the second light. This is Hering's 'three-image phenomenon,' and it has been studied afresh by Orbeli and Dittler (8). These experimenters find that the color and brightness of the second (compound) image depend on the color and intensity of both stimuli, while the third image depends on the color and brightness of the second stimulus alone. Thus if the foremost light is intense red and the second light a less intense green, a green after-image (the third image as described

above) will be had as the 'positive and like-colored' after-image of a stimulus whose primary image (the second as described above) has been altered to a dull red.

Goldschmidt (5) undertakes to measure the brightness of positive after-images by comparing them with the simultaneous primary image from a second stimulation. It seems incredible that this second stimulation should not appreciably modify the brightness of the after-image to be measured, and yet Goldschmidt states that it matters not at all whether the comparison stimulus is presented just before, simultaneously with, or just after the rise of the positive after-image. Other remarkable conclusions are: that visual and general fatigue do not effect the brightness of positive after-images although they do affect the general accuracy of an observer's judgment; that considerable differences in the state of adaptation of the eye have very little influence on the brightness of positive after-images (for a decided dark-adaptation the after-image is somewhat brighter); that the brightness of an after-image is almost independent of the color of the stimulus; and that if the brightnesses of different positive after-images are compared with simultaneously presented comparison stimuli, the relative difference threshold is found to be constant. It lies between one-fifth and one-tenth, *and* is three or four times greater than the relative difference threshold for two simultaneous primary images. Further, and more readily credible, conclusions are these: that the intensity of the stimulus increases more rapidly than that of the positive after-image; that the brightest moment of a positive after-image follows its stimulus more closely the intenser the stimulus is; that on a given day the judgments of almost every observer vary in the same direction from (above or below) that observer's own total average. The author ascribes this last to the effect of light and dark weather on the idio-retinal light, which in its turn importantly modifies the positive after-image.

Flügel and W. McDougall (4) publish further determinations of that which McDougall has previously called the 'action-time' of light, *i. e.*, the time of exposure which gives the maximum stimulation effect of a given light intensity. The action-times found, varying between 50 σ and 200 σ , are not significant because the intensities that were used were not measured. "When the action of light upon the retina is prolonged beyond its action-time the intensity of the sensation declines very rapidly, and more rapidly the more intense the sensation, the more intense sensation declining to 50 per cent. of its initial value in about one-fortieth of a second" (p. 206). Inas-

much as the intensity at any moment of a so rapidly changing sensation could not be judged with any accuracy, and from other statements of the authors as well, the reviewer supposes this to mean that the maximum intensity produced by an exposure that exceeds the action-time is less than the maximum produced by one of action-time length: if so, it follows that either the as yet ungiven continuation of a stimulus already exerts an inhibitory effect on the stimulation that has been given, or else the rapid decline in the sensation prevents the observer from *passing judgment* on what has been the maximum of the sensation. This latter is surely the more probable, and thus it should seem that the value of this investigation lies not in its bearing on action-time as related to brightness, but in its bearing on the process of (brightness) judgment. The authors state that the difference threshold is higher when the comparison is simultaneous than when successive; this they attribute to "falsification by physiological 'contrast.'" They mean 'simultaneous contrast,' and do not deem successive contrast to be so mischievous. It is further stated that in rod-vision the reduction of duration of a stimulus is equivalent, so far as the intensity of sensation goes, to reduction of the intensity of the stimulus, that is, the Talbot-Plateau law holds for rod-vision.

Although the field of a telescope or spy-glass never presents its objects so intense as the same objects are when viewed by the naked eye, yet the telescope field ordinarily, specially at twilight, looks brighter. Why? A. Müller (7) attributes this partly to the contrasting black border which is seen around the telescope field, but more specially to some considerations connected with the objective brightness differences given in the telescope field, and with Weber's law. These considerations are not given in further detail because they seem to the reviewer to be fallacious.

Babák (1), from work on amphibian eyes, finds evidence of 'qualitatively' opposed (antagonistic) retinal processes. Both light and darkness act as positive stimuli: and the retina when not exposed to light still influences, through the central nervous system, the tonus of the body, *et cætera*. In this connection Liesegang (6) describes a chemical combination which alters much more rapidly in darkness than in light, *i. e.*, which is 'affected by' darkness rather than by light. The process of change is described which this substance undergoes when exposed to darkness. Liesegang believes that this discovery bears interestingly on the question whether black is a sensation.

Winch (11) discusses at length the relation between the development of color terminology in children and the development of their sensibility to different colors. He finds the 'order of the correct

application of color-names' in children from three to five years old, to be: black-white, red, blue, green, yellow, violet, orange. He suggests that this is perhaps the order in which color sensation, at first unitary, gradually differentiates. Winch also presents (12) a study of the color preferences of London school children and others, as found by asking them to write down the *names* in their order of preference. The results are:

Youngest girls	(7 to 10 yrs.)	red	blue	yellow	green	white	black
" boys	" "	blue	red	" "	" "	" "	" "
Older girls	(10 to 15 yrs.)	" "	" "	green	yellow	" "	" "
" boys	" "	" "	" "	" "	" "	" "	" "
(21) Adolescent males	(15 to 20 yrs.)	" "	" "	" "	white	yellow	" "
(24) Men	(20 to 39 yrs.)	green	blue	red	" "	" "	" "
Women teachers	(18 to 46 yrs.)	blue	green	white	red	" "	" "

There is some slight indication that color preferences are more closely correlated with general mental development than with age irrespective of that development.

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VISION-COLOR DEFECTS

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The question of color tests is attracting considerable attention in England. Judged both from the general results of the tests and from the difficulty of deciding in specific cases whether a man is unfitted for service in the merchant marine because of color deficiency, the tests at present used by the Board of Trade must be very faulty or very badly conducted. *Nature* (2, 7) gives an interesting account of the case of Mr. John Trattles, who in six color tests during the last six years failed three times and passed three times, and was finally declared competent to act as second mate, by a special civil court, upon the testimony of fellow seamen who had made practical tests of his ability to 'pick up the lights,' against the unanimous verdict of a group of scientists that Mr. Trattles was certainly color-blind. The general efficiency of the tests may be judged by the statement made in the House of Lords during a debate (2) upon the need of an investigation of the tests used, that in the four years 1905-09 out of 25,151 candidates examined, 212 men were rejected, some of them upon a second test. As there seems to be overwhelming evidence that at least 4 per cent. of all white men are color blind, one would expect to hear of over a thousand rejections; and if the anomalous trichromates as well as the color-blind were excluded from such service, as in Germany, the number of rejections should be very largely increased. A committee to look into the whole matter of color tests is announced in *Nature* for June, 1910 (p. 529).

During the year a number of persons interested in the subject have published suggestions for improving the methods of color testing (3, 5, 9). The Holmgren test which seems (5) to have been the chief test used by the Board of Trade since its recommendation by the committee of 1890 (6) is attacked because it catches men who are not color-blind and fails to catch those who are (6), because it is illogical (5) and unjust, etc. Various lantern and out-of-door tests are recommended (6, 9, 15). Naming tests are judged to be fairer and surer than matching tests (9). Allen (1) recommends the method of persistency curves as a final test in disputed cases.

H. M. (11) suggests that color-blind seamen might be enabled to pick up the lights successfully if they were provided with colored glasses through which they could view signal lights, since through a red glass a red light could be seen while a green one would disappear, etc.

In his new *Manual of Mental and Physical Tests* (14), Whipple devotes a section to color-blindness, giving careful directions for the use of the Holmgren worsteds and the Nagel Tafeln.

Stilling (13) gives an illuminating discussion of the known anomalies of color vision from the point of view of pathological ophthalmology, and briefly outlines his general position in the introduction to the new edition of his *Pseudo-Isochromatische Tafeln* (12). From an extended comparison of many cases of congenital and acquired color-blindness, he comes to the conclusion that all color defects are pathological, the so-called congenital cases being the result of affections in the optic tract during early years, or during the foetal period. Congenital total color-blindness, which is practically identical with acquired total color-blindness, seems clearly pathological on account of the visual defects which accompany it (nystagmus for example); congenital partial color-blindness is closely analogous to acquired partial color-blindness, except that in the latter, visual acuity is more likely to be affected. Stilling finds it easier to prove this analogy in the case of red-green blindness, since that is so much better known; but he feels confident that congenital blue-yellow blindness (of which he has studied *nine* cases) will show a similar analogy to acquired blue-yellow blindness. The color anomalies which it has been customary of late to designate as anomalous trichromacy are either cases of color-blindness in which the subject has learned to distinguish many colors by secondary criteria, or cases of sub-normal sensitivity to red and green, a state of color amblyopia similar to that which often occurs before temporary red-green blindness. He lays special emphasis on the fact that most of the work upon anomalous trichromates has been conducted and described by persons who are themselves more or less defective in color vision.

Stilling highly recommends tests of color contrast for the determination of color-blindness, and the production of permanent color-blind equations by mixing pigments.

His comparative study of congenital and acquired cases leads Stilling to the following conclusions in the psychology and physiology of color:

1. There must be in the visual apparatus at least three systems of specific visual energy,—a brightness system, a red-green system and a blue-yellow system.
2. In normal color vision there are four fundamental colors,—two pairs of antagonistic colors.
3. In partial color blindness, the specific energies necessary for

one of these pairs are lacking, leaving a color system reduced to one pair of colors.

Köllner's study of acquired red-green blindness (10) leads him to conclusions which differ somewhat from those of Stilling. Köllner finds a fairly distinct and definite type of dichromacy in certain disorders of the optic nerve, but he does not think the defect so closely similar to congenital red-green blindness as Stilling claims. His experiments upon one case in which he watched the progress of the disorder from normal color vision to complete dichromacy, and his work upon many other cases in different stages of color deficiency, lead him to posit the following characteristics as typical of acquired red-green blindness: (1) a two color system whose warm curve is intermediate in form between that of protanopia and deutanopia, while the cold curve resembles that of normal color vision; (2) a series of color sensations like those obtained from the blue-yellow zone of the normal eye; and (3) a brightness distribution like that in normal daylight vision.

During the development of the defect there is, perhaps, an early stage similar to that found in anomalous trichromacy; the first stage of which he is sure, however, is a condition in which yellow and green are confused, but yellow and red distinguished; in the second and final stage, all the colors from red to the neutral band in the blue-green are confused, and distinguished only by differences of brightness. Acquired red-green blindness is, then, a 'reduction form' from normal color vision, like the familiar congenital forms of dichromacy.

Guttmann (8) reports that, in a series of experiments with the Helmholtz spectral color-mixing apparatus, four anomalous trichromates of the green-weak type regularly obtained blue negative after-images from all lights in the warm end of the spectrum. As they seemed to get gray after-images from pure green and its complementary purple given simultaneously, and yellowish after-images from blue and violet, he concludes that he has added another proof of the close relationship between anomalous trichromates and dichromates, though he confesses himself quite unable to give any theoretical explanation of the fact that in its reaction to spectral lights the anomalous retina appears to be trichromatic, while its after-images are those of the typical dichromate.

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¹ This book was not accessible to the reviewer. Attention is called to a review in *Nature*, 1910, 84, 262.

VISION—PERIPHERAL, FOVEAL, ETC.

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The literature on this topic for the year is scattered.

Stratton (4) discusses the localization of diasclerotic light, taking his start from Veraguth.¹ Veraguth claims that sensations aroused by light entering the eye through the sclerotic coat are often localized upon the side on which the stimulus falls. This was found to be generally true of the temporal side of the eye with the exception of a few cases in which, in addition to the sensation referred to the temporal side, there was a weaker sensation which was referred to the nasal side. It was not true at all of the nasal side of the eye. Here the localizations were made in the usual way to the opposite side of the field of vision. Veraguth established to his own satisfaction that the stimulation of the retina was in each case on the side on which the stimulus was given, for the effect was gotten when the incident light was tangent to the surface of the sclerotic. In this case, he reasons, it could not pass through the eye and stimulate the retina on the opposite side. He believes that we have here an indubitable departure from the usual cross-localization of visual impressions, and an argument against the nativistic theory of visual space. For, he

¹ Veraguth: Die Verlegung diaskleral in das menschliche Auge einfallender Lichtreize in den Raum. *Zsch. f. Psychol. u. Physiol.*, 1906, 42, 162-178.

reasons, if the space value of the impression inheres in the retinal element we should not get such different results from stimulating the same elements by light coming in the one case through the pupil and in the other through the sclerotic coat. He explains the localizations on the temporal side by the principle of biological utility. They are just what should be expected from this principle. The need of the organism is that the impressions be referred in the direction from which the stimulation actually comes. This would not be fulfilled were the localization made on the opposite side. The principle of utility has not worked itself out on the nasal side because so large a part of the field of vision on that side is shut off by the nose. Diasclerotic and diapupillary stimulation thus have each developed a different group of factors to determine their localizations. With some persons it is not improbable that both sets of factors are aroused by diasclerotic stimulation. In this way he explains the double localization which he noticed in some of his experiments.

Stratton finds that there are regions on the temporal side where the localizations are (1) to the temporal side of the field of vision, (2) to the nasal side, and (3) to both sides. The first begins immediately posterior to the edge of the iris and extends back, roughly speaking, to the ora serrata. There are no rods and cones in this region and the light passes through and stimulates the retina of the opposite side. The localization to the temporal side is as it should be, thus, by the principle of cross-localization. The second region lies still further back. The light here stimulates the rods and cones lying beneath its point of incidence, hence the localization is on the nasal side. The third region is found at the junction of the first two. Here we might expect the retina on both sides to be stimulated, hence a reference of the images to both sides of the field of vision. On the nasal side, he finds that all the localizations are on the temporal side of the field of vision. He accounts for the absence of regions in which the localization is on the nasal side in two ways. (1) The ora serrata extends farther forward on the nasal than on the temporal side. (2) Light entering anterior to the ora serrata, instead of passing through the eye and stimulating the retina of the opposite side, may strike the surface of the lens near its periphery and be reflected back to the nasal side posterior to the ora serrata.

Stratton sees in his results nothing necessarily contradictory to the principle of cross-localization.

Hubbard (1) describes "a curious secondary visual phenomenon resulting from a stimulation of the macular region." This phenome-

non may be observed with any feeble source of light viewed against a dark ground; but it is best seen when a patch of soft red color, such as is given by a ruby lamp wrapped with tissue paper, is observed in a dark room. When so viewed two diffuse streamer-like brushes of bluish tint are seen to spread laterally from the lamp,—to the right in the right eye and to the left in the left eye. The brushes enclose a neutral space irregularly oval in shape; and terminate at one end in the macula and at the other in the blind spot. At the blind spot they are narrow and line-like. They have their greatest breadth midway of their length. At the macula they are of medium breadth. The following points may be noted: (1) The clearest effects are given by a narrow stimulus aperture. (2) The brushes are always blue in color whether the stimulus be monochromatic or white light. (3) Their intensity increases with an increase in the intensity of the stimulus light and varies with its wave length. The effect is weakest at the extreme ends of the spectrum, is strong in the yellows, and reaches its maximum at orange-red. (4) The phenomenon strongly shows the effect of fatigue. With the eye fixed in one spot, the effect dies away in two or three seconds. The brushes are best seen when the light is turned on suddenly, in which case they flash out at once, beginning at the macula and extending quickly to the blind spot.

The writer concludes that there is some connection between the phenomenon and the distribution of the fibers of the optic nerve between the macula and the papilla. It would seem quite possible that certain excitations passing from the macular region along these fibers to the papilla are capable of inducing secondary effects in the portions of the retina through which they pass.

Rayleigh (2) reports an investigation conducted by him of the sensitivity of the eye to variations of wave length in the yellow region of the spectrum. He was prompted to this work by Edridge-Green's¹ method of determining the number of separate parts or divisions in the spectrum within each of which the observer can sense no color difference. Edridge-Green's method is as follows: Movable screens are provided in the focal plane of the spectroscopic telescope by means of which the breadth of the spectral section admitted to the eye is limited. Beginning at the extreme visible red, more and more of the spectrum is admitted, until a j.n.d. of color is obtained. This gives the first division. Starting again from the terminus of this division the whole spectrum is gone over. Working by this method the normal eye distinguishes about 18 of these divisions which Edridge-Green

¹ *Roy. Soc. Proc.*, B, 1910, 82, 458.

calls monochromatic. Edridge-Green uses this apparatus to test color sensitivity or its converse, color blindness. When tested by Edridge-Green, Rayleigh was able to distinguish only 17 changes of color tone in the spectrum. In the yellow for example a transition of 595-586 was required.

Rayleigh considers this method of testing color vision very insensitive. Working with the color box (after Maxwell) described by him in 1881,¹ he found that in the yellow region of the spectrum, he was able to distinguish as different the interval between the *D* lines which formed about one fifteenth of the division he called monochromatic by the Edridge-Green test. He points out two probable defects of the Edridge-Green test. (1) The gradual character of the transition is an obstacle to the recognition of the difference. There is a tendency for the next preceding color on which the eye has rested in going from one end of the division to the other, rather than for the standard color, to form one of the terms of each comparison. (2) Too small an area of the retina is stimulated by light of the same wavelength. The colored fields compared should have considerable area.

Scrini (3) discusses the 'mouches volantes.' His article covers the following points: (1) The literature is reviewed to the present time, beginning with Maître Jan, 1707. (2) Four sets of observations are given, selected from his own clinic as typical. Specks, lines, irregular blotches resembling dendrites, and patches made up of complications of a part or of all of these are described as seen floating before the eyes. (3) The phenomenon is discussed in relation to entoptic appearances (globules, fibers, etc.) ascribed to opacities in the humors of the eye.² By casual observation the connection is not obvious, but by a close analysis, the lines, specks, etc., characteristic of the phenomenon described by Scrini's subjects may be detached from the masses forming the entoptic appearances. (4) The phenomenon is also discussed in relation to neurasthenia and various nervous disorders accompanied by morbid introspective tendencies. Scrini points out from his own observation and from the literature that these appearances can be observed by practically every one when the observation is attempted under the proper conditions and after the subject's attention has once been drawn to them. The phenomenon, then, is normal. Two reasons are given for the frequency with which it is

¹ *Nature*, 1881, 25, 64-66; *Scientific Papers*, 1, 453. Vide also *Nature*, Aug. 18, 1910.

² Donne (*Arch. gen. de Med.*, 1830, 23, 113), for example, reports that opacities corresponding in shape to these appearances are found under the microscope in the aqueous humor of the dead eye.

reported by the neursathenic: (1) He has a hyper-sensitive retina, hence senses the phenomenon more strongly than the normal person. (2) His attention is morbidly predetermined towards subjective phenomena.

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HEARING

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The year 1910 has brought us at least two publications which set forth experimental results of prime importance for the psychology of hearing. It is interesting to note that both emanate from the Berlin laboratory, indicating that we must still look to this quarter for most of the major work being done in this field. One of these publications is by the director himself, a very painstaking study of the phenomena of combination tones (8), while the other, by Köhler, bids fair to bring about a small revolution in our conceptions of tonal quality (3).

Stumpf's investigations were inspired mainly by his various controversies with Krüger concerning the significance of difference tones for the phenomena of consonance. In considering his results it will be well to review briefly Krüger's contentions, the critical presentation of which, as it happens, has reached its rather long drawn-out conclusion within the year (4)—and almost simultaneously with Stumpf's renewed attack.

Krüger, it will be remembered, bases consonance in the main on the coincidence of two or more difference tones, provided that the total number of difference tones present must be relatively small. He thus prepares the way for a series of combinations which show all grades of consonance and dissonance, and also at times a condition of neutrality. For instance, he argues that the position of an interval in the scale, with reference to absolute pitch, introduces alterations in the 'sonance' character to such an extent that only a few

intervals retain their consonance intact when the pitch is varied. For most intervals a more or less limited region of the scale alone satisfies their consonance requirements, above that they tend towards neutrality, and below it towards dissonance. This fact he bases on physiological and other conditions which render the difference tones more effective in one region of the scale than in another.

Krüger's complicated analysis refers not merely to the difference tones of 'first' and 'second order,' which are commonly accepted. His investigations have led him to conclude that there are clearly perceptible at least five orders of difference tones. These may be derived by a process of continued subtraction which starts with the numbers expressing the relation of the interval and continues by taking each time the difference of the two smallest numbers among those of the objectives and difference tones present.

It is to this question of the five orders of difference tones that Stumpf now turns his attention. After an investigation which is a model for exact and delicate manipulation of instruments, and steady unbiased observations, he arrives at the following results: The formulæ for all possible perceptual combination tones may be thus stated: (1) $h-l$, $h+l$. (2) $2l-h$, $2h-l$. (3) $3l-2h$, $3h-2l$. (4) (probable) $4l-3h$, $4h-3l$.

In the above h signifies the higher objective tone, and l the lower objective.

These formulæ are limited, however, by the following facts: The only intensive difference tones are those expressed by $h-l$, when the objectives are within an octave, and $2l-h$, which only exists under this condition. Intervals within the minor third alone produce the difference tones indicated by orders 3 and 4 of the formulæ. Beyond the minor third there exist only the combination tones of orders 1 and 2. Above the major sixth the $2h-l$ is lacking; above the octave there remain only $h-l$ and the summation tone $h+l$; beyond the ratio 1 : 8 there is only the summation tone and beyond 1 : 12 there are no combination tones.

Judging from the nature of Stumpf's methods and execution, this would seem to dispose rather effectually of Krüger's elaborate hypotheses, and with them his attempts to base consonance exclusively on a perceptual basis. This is the more evident inasmuch as Stumpf's investigations indicate that combination tones are always directly derived from the objective tones, and not from beats nor, except in the highest ranges of the scale, from one another as Krüger maintains. Neither does Stumpf find any evidence for the construc-

tion of 'mean' tones among the difference tones; a phenomenon which, according to Krüger, helps to make an interval rough-sounding whenever two or more difference tones chance to be near one another in pitch.

As a striking culmination to the critical presentation of his own theory, Krüger cites Riemann's fatal objection to Stumpf's fusion theory. The objection is based on the apparent impossibility of explaining by this means the dissonance of the triad $c-e-g^{\#}$, wherein all the binary intervals are consonant, namely, two major thirds and a minor sixth. According to Krüger's theory the dissonance of this chord is clearly indicated by the character of the difference tones which are produced. Considering the tempered intonation, these are, indeed, all different and may well contribute to the unsatisfactory result. Krüger fails, however, to note that this same tempered intonation is responsible for considerable impurities in the several binary combinations. If we assume, as he does, $c' = 256$ vib., $e' = 323$ vib. and $g^{\#} = 406$ vib., we find the last note standing for either 403.75 vib. in its relation of $4:5$ with 323 vib., or as 400 vib. in the same relation to e' as the latter should be in just intonation (320 vib.); while, on the other hand, it must stand for 409.6 vib. in order to fulfill its relation of $5:8$ with c' . It is one thing to maintain that intervals which fuse naturally must continue to do so when they enter combinations with another related tone, but surely it is quite another thing to require that this shall hold true when the tone in question is a compromise between two tones each of which bears a distinct relation to each of the tones of the original interval. It would, of course, be impossible to produce this triad with the relations noted in just intonation, and it seems quite possible, to the writer at least, that the chord in tempered intonation is peculiarly suited to emphasize by contrast the unsatisfactory result of requiring one tone to do duty for two.

We are thus apparently thrown back upon some physiological disposition which may be regarded as conditioning the phenomenon of consonance, a supposition which has always been fundamental to Stumpf's theory and one which the writer, among others, has attempted to make plausible.¹ Not that difference tones and overtones may not be regarded as striking accessory features, as they doubtless are within their respective limits, but that any attempt to base the phenomenon of consonance on purely perceptual evidence would appear to be demanding overmuch of the perceptual complex.

¹ PSYCHOLOGICAL BULLETIN, 1909, 6, 297-303.

The most striking results of the year are undoubtedly those of Köhler (3). Köhler started his investigations with an attempt to analyze the vibrations of the ear drum. In the delicate operation of attaching a small mirror to the surface of the drum for the purpose of reflecting and photographing light images which would be projected when the ear was stimulated by sounds, he had remarkable success. Although the tracings which he obtained on the photographic plate were too indefinite to be subjected to any strict analysis in accordance with Fourier's law, they were still exact enough to indicate certain general qualitative and quantitative differences.

Among the various movements recorded he was able to distinguish clearly those due to pulse, head movement and swallowing from those which were introduced by the regular vibrations of a sound-producing agency. Of these latter he differentiates two: (1) a sudden and continued alteration in the location of the curve, which he refers to the reflex of the tensor tympani, and (2) the propagating vibrations which are due to the oscillations of the hammer. From a study of the first factor he is able to set aside the so-called 'accommodation theory' with respect to the tensor tympani, and prove that the tetanic contraction, which sets in immediately a tone is sounded, is entirely dependent upon the intensity of the sound and has no discoverable relation to the pitch, or vibration rate. The use of the tensor is thus to be understood as Johannes Müller conceived it—a purely protective means against too intensive movements. It appears, however, to function for all sounds, and not merely for the more intensive stimuli. From the greater resultant tension which is indicated when two tones sound together than when either sounds separately, he concludes, further, that each tone in a combination loses something of its intensity, a fact which is borne out by the observations of hearing.

We must regret that the method has not thus far succeeded in producing curves of sufficient definiteness to determine the presence or absence of difference tones in the drum. Further investigations may possibly bring to light the facts in regard to this much disputed assumption of Helmholtz.

The most interesting results of this analysis are those bearing on the quality of tones. It was found that the fundamental forms of curves produced by the same instrument sounding at different pitches remained fairly constant, thus substantiating, in the main, Helmholtz's theory of quality as dependent on the number and intensity of overtones present. The perceptual quality, however,

depends more on the *relations* which remain constant among the overtones, than on the inherent qualities of the separate tones, as Stumpf has maintained, for the reason that the qualities of the single tones vary with their pitch. But before he had extended his investigations of the 'interval qualities,' the author stumbled upon certain phenomena which caused him to turn his attention during the remainder of this contribution to the study of the qualities of separate tones.

In investigating the forms of curves produced by singing the vowel *o*, he found that the relatively constant form which the curve revealed at one pitch level, became markedly different when the pitch was lowered, and indeed, the more different did it become, the lower the voice was pitched. The remainder of the investigation, dealing largely with the problem of vowel sounds, bring some rather surprising results in that they show quite conclusively that the whole range of pure tones possesses unmistakable vowel qualities. The progression is definitely in the order *u, o, a, e, i*, each vowel having its own characteristic pitch which it resembles more nearly than any other. The observations to prove this were made by various, for the most part unmusical, persons who were required to judge as to the resemblance to the vowels of a series of 30 tuning forks, which ranged from 163 to 4,000 vibrations, and were sounded behind them in irregular order. There were no deviations from the result above noted. All the pure vowels were clearly referred to definite regions of the scale with the necessary transitions intervening.

Further investigation of a more precise nature for the purpose of discovering the optimal pitch for each pure vowel, indicated conclusively that the vowels range in intervals of an octave above each other, and their vibration rates approximate those of the *c's* in music, at concert pitch. In other words, *u* has an optimal vibration rate approximating 264 vibrations; *o*, 528; *a*, 1,056; *e*, 2,112; *i*, 4,224. An octave above the *i* transforms that vowel into an *s* or *ch* sound, while an octave below the *u* reproduces the rumbling *m*; *ö* and *ü* are not pure tonal qualities and do not appear in the series, whereas *ä* is a transitional tone between *a* and *e*. The most striking thing in these results, obtained by four independent observers, is their almost negligible deviations from each other.

It is further shown by these results that the 'formant theory' of Hermann, who has assumed that a special sort of sound at a distinct region of the scale was the necessary adjunct of each vowel, is quite superfluous—as well as indemonstrable. The vowel quality is directly

given in the pitch, and without the presence of the particular pitch the vowel is impossible.

An interesting side light is thrown upon this investigation by the brief report of Ranjard (7) on the cure of the deaf by treatment with the 'vowel-siren' of Marage.¹ This instrument is constructed to reproduce the characteristic qualities of the vowels, which, according to Köhler, are their characteristic pitches. Ranjard's results, which show improvement in the condition of 75 per cent. of the cases treated, indicate somewhat diverse effects in different cases on the improvement for vocal sounds, music and noise. Some cases, he says, show improvement for words alone, others for vowels but not for consonants, and in one case he succeeded in developing only the hearing for noises. If we accept Köhler's results, those tests would seem to bear out the vast importance of the vowel qualities for hearing in general, rather than the necessity of assuming three different central functions for vocables, music and noise as Ranjard concludes.

Bingham in his *Studies in Melody* (1) has made a positive contribution to this problem by his critical and experimental examination of the theories of Lipps and Meyer on the subject. After differentiating the problem of tonal relationship from the more fundamental problem of consonance, he proceeds to evolve a theory of esthetic unity based on the phrase, period or totality of the musical sequence. The principal factors in this unity he finds to consist in (1) the preference for the 'falling inflection,' *i. e.*, the lower of two tones; (2) the 'law of return,' or desire for the tone started from, whatever it may be; (3) the preference for an ending which is expected, and which (4) is usually one of the tones of the tonic chord, preferably the tonic itself. The Lipps-Meyer 'law of 2,' or universal desire for an ending on the tonic, thus becomes only a special case in the general "tendency of every interval, yes, of even a single musical sound, to establish a tonality attitude."

On the further assumption that this attitude is essentially motor, the author proceeds to investigate the effect of hearing musical intervals on the rate of finger tappings. The results exhibit a general correlation between the beginning of a tonal sequence and retardation in tapping, also a fair correlation between the end of the sequence and acceleration in tapping. The more particular and to be expected correlations between related intervals and acceleration on the one

¹ The construction of this instrument is described very briefly in *C. r. acad. d. sci.*, 1900, 130, 746-748. The data are insufficient, however, to determine the exact pitch of the several vowels which it is capable of producing.

hand, and unrelated intervals with retardation on the other hand, are very imperfect. It is also to be noted that the experiments were performed upon only six observers, and were apparently not repeated. Thus the tapping rates given do not represent averages, but single experiments. Furthermore the source of the tones was a reed organ of presumably tempered intonation. From the above it is not very evident that so remote a coördination as is shown by the tapping finger is calculated to enlarge greatly our knowledge as to the nature of so characteristically psychic an attitude as that upon which our sense of melody depends.

Hornbostel (2) has given us some critical remarks of interest on the methods of comparative musical studies. He considers in some detail the results of (1) direct experimentation on non-European subjects, (2) tonal measurements made on their musical instruments, and (3) investigations on phonograph records. With respect to the first of these methods, it would appear that we are more often measuring the individual's capacity of attention in our tests than his real musical ability, since the important effects of practice are almost invariably lacking. Regarding the usage by Orientals of intervals smaller than the semi-tone, we must remember that these intervals are not often used as such, but usually in connection with larger intervals. What we must grant is the remarkable acuity for remembering a greater number of normal intervals than we use in our music. The examination of foreign musical instruments is greatly hampered by the necessity of keeping the instruments in tune, except in the case of pan's-pipes and the xylophone. Investigations on these instruments indicate a very high degree of differential sensitivity for the unusual range of intervals employed, since both in the case of Brazilian pan's-pipes and Siamese and Javanese xylophones the comparison of different instruments reveals a constancy of the intervals which can hardly be referred to mathematical knowledge or to the aid of physical instruments in their tuning. With respect to the melody problem we are told that we must not be prejudiced by our own current musical notions, but remember that primitive music is largely independent of harmony. It is thus the total form of the melody, not the mere tones or intervals, but the *motives* which involve changes in intensity, rhythm, relative duration, tempo and quality in the widest sense, which constitute the melodic significance of non-European music, — a conclusion to which Bingham would doubtless heartily subscribe.

Among the minor contributions of the year, the Peterson-Titchener controversy as to the seat of combination tones (6, 10), brings into

review most of the recent literature on the subject, but leaves the question still open as to whether the middle or inner ear is more largely responsible for these phenomena. Lasareff (5) has shown how the König wave-siren may be easily adjusted so as to produce phase differences in two simultaneously sounding tones, and states that these differences in phase have no appreciable effect upon the tonal sensation, thus contradicting König and supporting the view of Helmholtz. A simple device for measuring quantitatively the intensity of sound in auditoriums has been described by F. R. Watson (11). It consists essentially of a telephone receiver connected in series with a thermo-galvanometer. The device is portable and easily adjusted, and with the aid of resonators attached to the telephone receiver, appears to be well adapted for registering weak sounds.

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DISCUSSION

THE JAMES-LANGE THEORY OF THE EMOTIONS AND
THE SENSATIONALISTIC ANALYSIS OF
THINKING

Professor Titchener's latest discussion of the James-Lange theory of the emotions¹ seems, to the writer at least, to form an interesting parallel to some phases of the recent controversy over the analysis of thought processes. In his study of the emotions James "brought us back to the crude and raw of actual experience";² Titchener has given us³ a description of the way in which an 'imaoinal mind' does its thinking—an account unexcelled in clarity ~~and v.~~ ness, which ranks with the James emotion chapters in power of stimulating the reader to careful detailed introspection. Yet James, in his emphasis on the organic factors in emotion, overlooked the elemental affective consciousness itself, and not a few psychologists to-day hold that Titchener and others,⁴ in emphasizing the sensational constituents of thought, leave out of account its non-sensational constituents, its 'meaning aspect'⁵ or 'relational elements'.⁶ Further, several of Titchener's arguments⁷ against the James-Lange theory of the emotions may be turned, almost as they stand, against the purely sensationalistic analysis of the thought processes. Such a turning of the argument against this later phase of sensationalism is the purpose of the present discussion.

On the negative side, Titchener points out (p. 481) the inconclusiveness of the argument that because I cannot experience or imagine an emotion without organic sensations, therefore the emotion must consist in organic sensations. Similarly, of course, as Calkins⁸ and Angell⁹ have already shown, a disproof (if such be possible!)

¹ *Text-book of Psychology*. 1910. Part II., pp. 474-484.

² *Ibid.*, p. 479.

³ *Experimental Psychology of the Thought Processes*. 1909. Cf. esp. pp. 7-22; 184-186.

⁴ Cf. esp. Colvin: A Marked Case of Mimetic Ideation. *PSYCHOLOGICAL BULLETIN*, 7, 1910, 260-268.

⁵ ANGELL: Lecture delivered at Columbia University, Feb. 3, 1911; also *Psychology*, Ch. X., esp. pp. 245-249 (4th edition, 1910).

⁶ CALKINS: Professor Titchener on the Thought Processes. *PSYCHOLOGICAL BULLETIN*, 7, 1910, 293-297; *Introduction to Psychology*, 1901, pp. 130 ff.; *A First Book in Psychology*, Ch. VIII., Appendix, Sec. VIII., 362 ff.

⁷ *Text-book*, pp. 481-482.

of the existence of 'imageless thought' carries with it no disproof of 'meaning aspects' or 'relational elements' of consciousness as non-sensational. The writer, for one, never caught an 'imageless thought' in her own mind, but seems very clearly to herself to have caught non-sensational content along with the sensational, not only in 'thinking' proper, but also in imagining and perceiving.

On the positive side, Titchener gives two main arguments against the sensationalistic theory of the emotions. In the first place, he appeals to the testimony of introspection. A group of organic sensations is and remains just a group of organic sensations. Something more must be added to such a group to make of it a true emotion, and that something more is found in the affective elements of pleasantness and unpleasantness. In the same way introspection seems to many psychologists to show that a complex of images and sensations is and remains a complex of images and sensations; that something more—call it 'relational elements,' 'meaning aspect,' or what you will—must be added before it becomes 'meaning' or 'thought.'

In the second place, Titchener says, emotion and organic sensation are not the same thing, because the two vary, to some extent at least, independently. But the same is true, by Titchener's own showing, of images and 'meaning' in our thinking. Emotion and organic sensations vary independently of each other in their intensity, their quality, and their temporal relations.⁸ With regard to the temporal rise and fall of meaning consciousness introspection is extremely difficult, and whether or not such meaning consciousness can be said to have an intensity is doubtful. But the consciousness of meaning certainly seems to have both clearness and quality, or kind, and to both of these attributes the variability argument may be applied.⁹

With regard to clearness, just as "there are a quiet joy and a boisterous joy," but "there is no evidence that the former is less intense than the latter,"¹⁰ so for one person the sensational content of the "feeling of but" may be "a flashing picture of a bald crown . . . combined with an emphatic attitude,"¹¹ while for another it may be merely a vaguely localized fleeting kinesthetic image. That is to say, the sensational content for the first person is much clearer and more complete than for the second. Yet I think that even the sensationalists would hesitate to say that the meaning of 'but' is any less clear for the second person.

⁸ *Text-book*, p. 482.

⁹ CALKINS: *PSYCHOLOGICAL BULLETIN*, 7, pp. 296-297.

¹⁰ TITCHENER: *Experimental Psychology of the Thought Processes*, p. 185.

Again, to apply the independent variability argument to quality, or kind, of meaning-content and sensation-content: for Titchener 'meaning' itself is "represented by the blue-gray tip of a kind of scoop . . ."; for one of his students it is the "mental unrolling of a white scroll"; for another "a horizontal line with two short verticals. . .".¹¹ Must we then conclude that for each of these persons the consciousness of 'meaning' is an entirely different thing? It seems hardly possible, for, as Miss Calkins has pointed out,¹² they understand each other when they use the word. The logical conclusion is then, that the *kind of image* content is itself unimportant, so long as it 'represents' or 'carries' (I think the second of Titchener's terms a very exact expression of what seems to happen) the real meaning-content—in this case the relational consciousness of *withinness*, which is common to all these different varieties of the consciousness of 'meaning.' In other words, just as Titchener concludes that "the sensations of the organic reaction cannot be identified with the emotion,"¹³ so the same kind of reasoning shows that the images which 'carry' the meaning-content cannot be identified with the meaning-content.

This conclusion is further corroborated by the fact that the independent variability of image-content and meaning-content holds not only for different persons, but for the same person at different times. If I may illustrate from my own case, I am, like Titchener, of a strongly imaginal 'mixed' type, with visual and kinesthetic imagery predominating. It is often, therefore, relatively easy for me to catch the sensation-image-content that 'carries' the meaning-content. But the puzzling thing, if the sensationalists are right, is that the image-content, when caught, turns out to vary so much from time to time, even when the meaning-content is the same. Take, for example, that same case of the consciousness of 'meaning' itself. Observations carried on during only a few days gave the following list of 'carriers' of the meaning consciousness:

1. A box-image suggestive of one mentioned by Titchener,¹⁴ but more detailed, and visual as well as kinesthetic. In its most complete form a whitish box seen from above and a little to one side, with the lid half open showing a darkish interior. This visual image was sometimes relatively detailed, sometimes sketchy almost beyond recognition. Along with it there were more or less distinct kinesthetic

¹¹ *Ibid.*, p. 19.

¹² *PSYCHOLOGICAL BULLETIN*, 7, p. 296.

¹³ TITCHENER: *Text-book*, p. 483.

¹⁴ TITCHENER: *Psychology of the Thought Processes*, Appendix, p. 213, note 24.

images, either of bending over and peering in (images located in back of neck and eyes), or of lifting up the lid (an image in hand and arm). Sometimes one kind of kinesthetic imagery was present, sometimes the other. I never detected both on the same occasion.

2. A visual image of a pair of eyes looking into mine. Color and surroundings usually indefinite, but I seem to be looking into and beyond the eyes. Kinesthetic imagery or sensations such as usually accompany visual attention.

3. Visual image of word 'meaning.' Sometimes on a gauzy, half transparent background, again standing out in space, half concealing space behind. No kinesthetic imagery to be detected.

4. Visual image of eyes as in 2, but with the eyes looking toward my right. Kinesthetic tendency to turn my own head and eyes in same direction.

5. Visual verbal image, without the transparent background, but *with* a kinesthetic image (head and eyes) of looking away from word toward the right. The visual image often very sketchy and fleeting.

6. Sketchy visual image of sign-post or arrow, pointing away from me and toward the left. Kinesthetic image of looking in direction indicated.

7. A tactal-motor (often also auditory) verbal image. This occurred in combination with any one of the other six images, but even this could not be detected as invariably present. It often preceded and was replaced by one of the other images. On the two or three occasions when it occurred alone it 'carried' no 'meaning-content' at all, and I discovered that while saying the word over to myself my attention was really directed elsewhere.

Here are, then, six different sets of images as carriers of the meaning of 'meaning.' Must the meaning-content be different in all six cases? The answer of introspection is prompt and emphatic. 1, 2 and 3 all carry exactly the same meaning-content—meaning is that which is within something else. The relational consciousness of 'within-ness' is somehow present in each case, as it is in the examples which Titchener gives. In 4, 5 and 6, on the other hand, the meaning-content is somewhat different—meaning is that which is pointed at by something else. The relational part of this consciousness might perhaps be indicated by '*from x to y*', where *x* and *y* are images which may themselves vary, but which always stand in the *from-to* relation. To these two different kinds of relational consciousness, the *within-ness* and the *from-to-ness*, correspond the two slightly different sig-

nifications which the word 'meaning' has for the observer. It will be noticed that whereas in 4, 5 and 6 the kinesthetic imagery, like the meaning-content, is practically the same, in 1, 2 and 3 there is no one kind of sense-content that is constant. It seems hardly possible, therefore, if this introspective record is correct, to hold that the meaning-content common to all three can be identical with any of the sensory-imaginal-content.

Examples might be multiplied. The above shows an instance in which a probably non-spatial relation or meaning is 'carried' in spatial terms. There are other cases where definitely spatial meaning-contents seem to include non-spatial relations. The verb 'to replace,' for example, means to me one of two things: it may mean to put an object back in the same place where it was; or it may mean to put an object in a place formerly occupied by another. The image-content of each of these two meanings is varied, depending largely on circumstances and especially on context. But the meaning-content is for each meaning constant. It includes, in Miss Calkins's terminology, the 'relational element' of *in-ness* (probably spatial), the temporal relations of *now* and *before*, and the non-spatial, non-temporal relations of *same* and *other*. In the first meaning of 'replace' it is the 'same' thing (image of whatever kind it may be) that is in the 'same' place (usually visual-kinesthetic image), but the 'in'-ness is 'other' than 'before.' For the second meaning of the word, it is an 'other' thing in the 'same' place as 'before.'

I do not wish to give the impression that all the images which carry the meaning of words and sentences are for me variable, for quite the contrary is the case. Not only have I a relatively fixed number-form, days-of-the-week form, and months-of-the-year form of which I make constant use, but the image-content of many words and phrases seems to vary from time to time only in clearness and detail, not in quality. Thus my own consciousness of 'but' is carried by a fleeting image of hand-movement closely connected with the explosive character of the tactal-motor-auditory image or sensation of the word. And my consciousness of 'essence' is carried by a visual image of a few dark layers or folds (of covering or drapery) to the right of and over (apparently partially covering) a pinkish blotch, accompanied by a distinctly unpleasant tactal-motor image which seems to be a remnant of shivering or shrinking. Whether the affective toning is due to the idea of 'bare essence,' or is a reminiscence of earlier struggles with Hegel's Logic, I do not know. At

any rate, practically the same sensational-imaginal-content occurs whenever I think of 'essence.' But I do not believe, in spite of that fact, that my consciousness of the meaning of 'essence' is any more definite or more stable than is my consciousness of the meaning of 'replace' or of 'meaning.' *Im Gegenteil!* Yet the image-content of 'meaning' is variable, while the image-content of 'essence' is constant. The testimony of my own introspection, then, would be that meaning-content cannot occur alone in consciousness without any sensational or imaginal content. Yet the two are not the same thing, but the meaning-content which is 'carried by' image-content is just that relational consciousness which introspection seems to find over and above, though intimately woven in with sensations and images.

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A CORRECTION

TO THE EDITOR OF THE PSYCHOLOGICAL BULLETIN:

Will you permit me to express my regret for two misprints in my first quotation from Titchener's *A Text-book of Psychology* on page 17 of the January number of the BULLETIN. The word 'plane' is printed for 'phase,' and asterisks are omitted before 'human experience.' Through a mistake—not that of either printer or publisher—the proof never reached me and therefore was not compared with the texts of the authors whom I quoted.

MARY WHITON CALKINS

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SPECIAL REVIEWS

QUESTIONED DOCUMENTS

Questioned Documents. ALBERT S. OSBORN, Examiner of Questioned Documents. With an Introduction by Professor JOHN H. WIGMORE. Two hundred illustrations. Rochester, N. Y.: The Lawyer's Coöperative Publishing Co. Large 8vo. Pp. 501.

The attention of psychologists should be directed to an important contribution along one of the many by-paths that now cross their route, and which one or another may be interested to follow when on professional service bent. The well-written, and equally well printed and embellished volume by Mr. Albert S. Osborn, dealing with the problems of genuineness involved in 'Questioned Documents,' is a valuable compendium of the data and methods of the study of handwriting for evidential purposes. This specialized art develops and establishes its technique; and to this Mr. Osborn adds original contributions, particularly in the application of photography to the revelation of obscure detail, which commonly was not intended to be revealed or revealing. His use in appropriate cases of the stereoscopic photograph as a convincing document is striking. The range of topics is very wide and makes one appreciate how large must be the base of the wedge that is directed pointedly to the precise deciphering of the most intimate of manual records. The fading of ink and aging of paper; the influence of the instrument and kind of pen or point; the changes of age; the vagaries of caprice; the variations of accident; the evidences of tracing, of erasing and alteration; the bold forgery that varies too far and the slavish one that holds too near the original; and again the refinements of the measuring apparatus; and the tricks of the trade and the rival ingenuity of the detective;—these are but a few of the phases that enter into the legal and technical aspects of handwriting, and are all admirably presented in this volume. It goes beyond handwriting in applying similar methods to so impersonal an instrument as the typewriter, which likewise has been called into question as yielding an evidential document. The make, age, pattern and individuality of the machine may be revealed by enlargements and analysis.

But apart from the utility of the volume to lawyers and others professionally concerned in establishing the genuineness or falsity

of disputed documents, the treatment carries direct significance to the study of handwriting as a psychological record. Handwriting thus becomes the most brilliant example of the subtle equilibrium between the delicate fluctuations of deliberately intentioned factors, and the subconscious accompaniments that shape the finer issues. We must intend, but not intend too much or too little. The stage laugh or the signalling or nervous cough reveal their unnatural origin through the same mechanism. But the visual record remains for the minute analysis that goes beyond impressionism, while yet it supports its verdicts. To execute naturally involves not too deliberate an intent or too emotional an interest, yet cannot dispense with these wholly. Yet again: handwriting as a 'document in individual psychology proves at once the subtle variations of which individuality is the complex issue (and incidentally confirms the difficulty of generalization and the limitations of a rough and ready impressionism) and the minuteness of differentiation that separates the genuine from the forgery. The graphological records, with their deadly parallelism of photographic enlargements, become quite as significant illustrations of the psychomotor mechanism as are the kymographic or automatographic records obtained in the laboratory in the pursuit of special problems. In one aspect they are more valuable, in that they are the issues of real motives and situations, which is an essential part of the complex of which they form the unwilling but enforced witnesses.

Such minutiae of handwriting may be viewed as the data of a psychoanalysis developed pointedly in the interests of related problems; for here too the submerged mental procedures and affective and suppressed intents are to be objectively disclosed. Like all forms of such analysis, there is the difficulty of the technique and the uncertainty of the diagnosis and the interpretation. But these difficulties impart the fascination to the art, and serve to emphasize the bearing of the psychological principles involved. The interpretation of handwriting as a divining art is an old one. It was ambitiously exercised, not confined to the mere revelation of individuality, like the thumb print of modern days; but exploited to set forth the character of the writer and the ordinary range of his qualities. It was approached like palmistry or physiognomy as an art that was free to read character in line of palm or forehead or pen; and its practitioners were ready with final readings and convincing demonstrations. Yet for the advancement of knowledge such cultivation yielded little profit and a large crop of weeds. Transfer the interest, and refine

and concentrate the methods; and its returns though modest become secure. The older investigations of Pfreyer and the newer ones of Binet may be cited as illustrations of single aspects of the problem. The rationalization of the laboratory carries far beyond the limits of its *Arbeiten*.

As supplementary to the studies of the laboratory and approaching the problem from other interests and purposes, this study of the psychomotor mechanism as revealed in handwriting contributes a welcome illumination. Its value is due to the expertness of Mr. Osborn and the care and originality of his work. For a psychological library that aims to supply the study-material of related disciplines and arts, this volume is most suitable.

JOSEPH JASTROW.

UNIVERSITY OF WISCONSIN.

BOOKS RECEIVED DURING FEBRUARY

CLAPARÈDE, ED. *Psychologie de l'enfant et Pédagogie expérimentale*. Genève: Kündig, 1911. Pp. viii+471. Fr. 4.

HEYMANS, G. *Die Psychologie der Frauen*. Heidelberg: Winter, 1910. Pp. viii+308. Mk. 4.

INGEGRNIEROS, JOSÉ. *Analés de psicología trabajos del año 1909*. Buenos Aires: Etchepareborda, 1910. Pp. 353.

BERGSON, HENRI. *Matter and Memory*. Authorized translation by NANCY MARGARET PAUL & W. SCOTT PALMER. New York: Macmillan; London: Swan Sonnenschein, 1911. Pp. xx+339.

TELESII, BERNARDINI. *De Rerum Natura*. I. A cura de Vincenzo Spampinato. Modena: Formiggini, 1910. Pp. xxii+322.

DROMARD, GABRIEL. *Essai sur la sincérité*. Paris: Alcan, 1911. Pp. iv+243. Fr. 5.

FLOURNOY, TH. *Esprits et Médiums. Mélanges de métaphysique et de psychologie*. Genève: Kündig; Paris: Fischbacher, 1911. Pp. vi+561. Fr. 7.50.

ANONYMOUS. ("ELIZABETH MORISON and FRANCES LAMONT.") *An Adventure*. London & New York: Macmillan, 1911. Pp. vii+162. \$1.25 net.

A Hand Book of the Millennium according to Oahspe the Cosmic Bible. Los Angeles: C. L. Brewer, 1911. Pp. 63.

KOSTYLEFF, N. *La crise de la psychologie expérimentale. Le présent et l'avenir.* Paris: Alcan, 1911. Pp. 176. Fr. 2.50.

MÉNARD, A. *Analyse et critique des principes de la psychologie de W. James.* Paris: Alcan, 1911. Pp. 467. Fr. 7.50.

BALDWIN, JAMES MARK. *The Individual and Society, or Psychology and Sociology.* Boston: R. G. Badger, 1911. Pp. 210. \$1.50 net.

Memorias do Instituto Oswaldo Cruz. Ano 1910, Tomo II, Facic. 1. Rio de Janeiro: Manguinhos, 1910. Pp. 146.

CARUS, PAUL. *Truth on Trial. An Exposition of the Nature of Truth, Preceded by a Critique of Pragmatism and an Appreciation of its Leader.* Chicago: Open Court, 1910. Pp. 138. \$1.

NOTES AND NEWS

Professor Irving King, of the University of Iowa, will give courses next summer in the University of Wisconsin on the 'social aspects of education' and on 'principles of education.'

Professor E. B. Titchener, of Cornell University, gave a course of lectures in February at the University of Iowa. While in the west he also lectured at the University of Kansas and the University of Indiana.

The following item is taken from the press:

It is announced in the *Revue scientifique* that Professor Hans Meyer has presented 150,000 marks to the University of Leipzig for the inauguration of an institute of experimental psychology.

